Reptiles.—Mr. Boulenger figures in the proceedings of the Zoological Society of London the *Python curtus* of Hubrecht, a species long overlooked, but which is widely distributed in Malaysia, continental and insular.

M. F. Bocourt describes and figures in the last number of Mision Scientifique de Mexique the Colubrine snakes of that country, including Pityophis, Spilotes, Coluber, etc. The plates are admirable.

Prof. E. D. Cope describes, in the late Proceedings of the U. S. National Museum, the reptiles and Batrachia obtained during the voyage of the U. S. Fish Commission steamer Albatross around S. America to the west coast of N. America. He rehabilitates the genera Batrachyla Bell, and Nannophryne Gthr., which were founded upon species from Patagonia, describes a new Zachænus from that country, and a new Phyllodactylus and Tropidurus, each from the Galapagos Islands. He shows that in Tropidurus it is the females that are more brilliantly colored than the males, contrary to the rule in the allied genera Sceloporus and Liocephalus. The colors are red, while in Sceloporus they are blue.

Mammalia.—In the Bulletin of the American Museum of Natural History of New York, Dr. E. A. Mearns, U. S. A., describes a number of new Mammalia from Arizona. They are as follows: *Sciurus hudsonius mogallonensis, Fiber zibethicus pallidus, Hesperomys leucopus arenicolus; Sigmodon hispidus arizonæ; Lepus allenii; L. melanotis, Dipodomys merianii; D. chapmanii; and Cynomys arizonensis.*

EMBRYOLOGY.

Prof. Weismann on the Transmission of Acquired Epilepsy.—The chief instance favoring the transmission of acquired characters which Prof. Weismann finds difficulty in explaining is evidently the series of experiments with reference to artificial epilepsy in guinea-pigs performed independently by Brown-Sequard and Obersteiner. I judge that Prof. Weismann himself regards this as the case presenting the greatest difficulties for his theory, since he has treated it in particular twice in the Essays upon Heredity, summarily in pages 81-82, and at length in pages 310-319, written in 1887, and also since he refers to it as "the only definite instance which has hitherto been brought forward in support of the transmission of acquired characters." (P. 319.) Prof. Weismann acknowledges the results of the experiments to be

that various lesions of nervous substance in guinea-pigs are followed by epilepsy, and the offspring are often affected with nervous disorders, and sometimes with epilepsy. He defends his theory in two ways, first, by referring to infection as the true cause, and not heredity (p. 82, pp. 312-315); secondly, by pointing out the difficulty of conceiving the method of transmission upon either a preformation or epigenetic theory (pp. 315-319). Both these lines of argument are, it will be noticed, in the region of hypothesis and supposition.

As to the first point, there is no distinct evidence, as he himself admits, that epilepsy is caused by bacillus; and, in fact, in some cases it cannot be (p. 314), and yet he conceives that the transmission is by a bacillus from an infected part reaching to and attacking germ-plasm. The question here, as often elsewhere in the Essays, is resolved not so much into a matter of fact as a matter for conception. But how does the bacillus hypothesis simplify the problem for the imagination? If the microbe reaches the germ-cell how can it attack it any more than a "molecule of the brain of an epileptic animal?" (p. 310). If it could attack the germ-plasm, we have still the same fundamental difficulty as with transmission of acquired character, namely, as to how the bacillus can excite in the germ, not epilepsy itself, which is impossible, but such a peculiar disturbance of some peculiar molecular order that epilepsy will result after the many stages of evolution in a certain part of a certain tissue in the developed animal. That is, we must bring against the bacillus theory the same objection which he brings against the epigenetic when he asks how the germ-plasm can receive, "not indeed the peculiar structure of the stage itself, but such a molecular constitution as will ensure the ultimate appearance of epilepsy in the offspring." (P. 318). Prof. Weismann concludes that there is transmission, but one "which cannot depend upon heredity, and is in all probability due to infection." But if this method of infection is admitted, do we not have here, in a wide but true sense, heredity, a real transmission of acquired character, the acquired infectious (?) disease is transmitted to offspring; the offspring alike inherit it, whether we suppose the method of transmission to the sperm or germ-cell be by transference of bacillus, or of diseased molecules, or of gemmules, or any other way. By acknowledging that the continuous germ-plasm is affected, no matter how, but by or through acquired character, so that the character reappears in offspring, does not Prof. Weismann really concede the point at issue?

With reference to the second point, the *reductio ad absurdum* of other theories, it is quite possible to urge against the continuity theory

itself many of the same objections which Prof. Weismann brings against the gemmule and epigenetic hypotheses. The inconceivability as to number and complexity of gemmules is quite matched by the inconceivable complexity of a germ-plasm, which in the sixth generation is composed of 32 ancestral germ-plasms, in the tenth generation of 1024, etc., even if the reduction by one-half be accomplished for each generation as explained pp. 357, ff. Moreover, must not each of these germ-plasms be divided by the vast number of ova or spermatozoa which appear in any individual's lifetime, and which are all potential for a possible conception? I am not aware that Prof. Weismann treats this point; but at any rate it is difficult to see how, upon his hypothesis, we can escape getting down within a few generations to the ultimate unit which "cannot be divided without the loss of its essential nature." (P. 357.)

The fundamental difficulty, the causing from a remote part of the organism such a peculiar effect upon the germ that the disease reappears in the developed organism, this problem is unsolved by the bacillus hypothesis, as also by the gemmule and epigenetic hypotheses. But do we need these hypotheses? This must be granted, that the germ-plasm, whether continuous or not, is a living being within the body and also of it (cf., pp. 103, 170, 267, *et passim*); that the law of interdependence of function—disorder in one function exciting sympathetic affections in others—applies to reproduction; that farther, the reproductive function is specially sensitive to nervous disease, as nervous exhaustion, for instance, which causes mutilated spermatozoa, and so weakens for future development. Moreover, specific causes will produce each its particular effect upon the germ for its whole development. But does this sympathetic affection need to be conceived as actual transference of bacillus or gemmule? May it not rather be regarded as a dynamic response in a dynamic whole, a transference of abnormal motion rather than matter? It seems probable that epilepsy, for example, could cause specific changes in the germ, but this may not be heredity, which is a specific effect of such a nature as to bring about this special disease in the offspring after many and diverse stages of evolution. It is certainly most difficult to conceive how this telepathy can be secured, but it is always to be remembered that at bottom it is a question of fact rather than conception. By a series of experiments we must trace the effects of nervous disorder upon spermatozoon and ovum, and how this effect is carried through conjugation and development.

It would be rash in our present knowledge to say that all influences made upon the germ are of the transmitting type; but as to this case of artificial epilepsy we may affirm that such evidence as we have points to general and even special heredity of acquired character through some affection of the germ produced by that character. A nervous disorder, epilepsy, tends to produce in offspring some nervous disorder. It is plain that if there is any effect upon the offspring from this disease, it is upon the nervous tissue rather than upon any other tissue or function, and this deserves to be called general heredity; and so far as the special disorder is communicated, this may be called specific heredity. If a nervous disease tends to produce in offspring nervous disease of any kind, this is so far heredity.

It seems to me, then, that Prof. Weismann's theory of non-transmissibility of acquired character fails even when tried by his own presentation of this test case, but it is certainly to be desired, as he intimates (p. 82) that the series of experiments should be carefully and thoroughly followed up. We certainly owe much to Prof. Weismann's hypothesis, but it is not too much to say that it is still unproved in point of fact, and unsatisfactory as yet to the scientific imagination, at least so far as artificial epilepsy is conserved.—HIRAM M. STANLEY.

ARCHÆOLOGY AND ETHNOLOGY. [1]

Prof. F. W. Putnam, Curator of the Peabody Museum of American Archæology and Ethnology, in Cambridge, closes his last report in the following manner:

"Thus there are the following elements to be taken into consideration in any endeavor to trace the present North American tribes and nations back to their origin. First, small, oval-headed, paleolithic man. Second, the long-headed Eskimo. Third, the long-headed people south of the Eskimo. Fourth, the short-headed race of the southwest. Fifth, the Carib element of the southeast. All these elements must be studied with their differences in physical characteristics, in arts and in languages. From a commingling of all, with greater or less predominance of one over the other, uniting here and subdividing there, through many thousand years, there has finally resulted an American people having many characteristics in common, notwithstanding their great diversity in physical characteristics, in arts, in customs and in languages. To

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